

date this specific name and it is, therefore, to be credited to him. As stated above, this name falls into synonymy under the earlier established name *quercus*.

Considerable damage was done by the above locust during the past season near New Lisbon, N. J. Concerning them and the damage they did Mr. H. B. Scammell writes as follows: ". . . they infested the oak woods near New Lisbon, N. J., to an alarming extent. Some of the tallest oaks were nearly defoliated . . . The area infested approximated two square miles." Macropterous individuals and nymphs of both sexes were received from the collector for determination but no brachypterous specimens were sent. However, it is quite likely that both long and short winged forms were concerned here as in the case of injurious occurrence reported by Mr. Davis in 1912, Ent. News, Vol. XXIII, p. 2.

SOME TIPULID SYNONYMY.

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The following notes are published as a sequel to those given by me in the *Annals and Magazine of Natural History* (8) XI, p. 578-584.

1. In the *Canadian Entomologist* 1913, p. 200-205, Alexander has described two Japanese species under the names *Dicranomyia japonica* and *D. nebulosa*. From the long subcosta and the structure of the male forceps it is clear that these two species belong to the genus *Limnobia*. Moreover, in *japonica*, the radial cross-vein is considerably removed from the tip of R_1 , a structure which, as pointed out by Osten-Sacken, never occurs in *Dicranomyia*. Edwards (in letter) agrees with me as to the systematic position of these species, and I think there can be little doubt that *D. eiseni* Al., too, is a *Limnobia*. *D. gloriosa* and *lutzi*, both described by Alexander, should in my opinion also be placed in *Limnobia*, at least until the forceps in the still unknown males proves to be of the *Dicranomyia* type. Alexander's conception of these two genera seems to be so different from that of Osten-Sacken and all

other authors that an explanation how he distinguishes them is much to be desired.

2. In the above mentioned paper in the Annals and Magazine of Natural History (p. 580) I said that the palpi in *Aporosa* are placed at the apex of the rostrum. Not having at hand the scarce work of Webb and Berthelot, where Macquart first described *Aporosa*, I relied on Enderlein's statement, but Edwards, who has seen the work, has informed me that the palpi are removed from the apex of the rostrum and that Enderlein apparently had mistaken the "labellæ" in Macquart's figure for the palpi. *Aporosa*, therefore, is a synonym of *Geranomyia* Hal., as stated by Osten-Sacken.

3. Edwards (in letter) shares my opinion (l. c., p. 581) that *Liponeura* Skuse (incorrectly written "*Leiponeura*") is a good genus, but he has called my attention to the fact that the name is preoccupied by Loew for another genus of Diptera Nematocera. I, therefore, propose the name *Lipophleps* for it. Edwards does not think that *Lipophleps* is allied to *Atarba* O. S., as he has satisfied himself that *Atarba* has spurred tibiæ, a point of which Osten-Sacken was uncertain. If we attach the same importance as Osten-Sacken to the presence of tibial spurs, *Atarba* would be exceedingly difficult to locate systematically, for it can under no circumstances be placed in the *Cylindrotominae*. Alexander has, however, shown by several examples that this character has been overrated, and it is known that the generic characters taken from the tibial spurs in the order Trichoptera cannot be relied on. Although Alexander (PSYCHE, 1913, p. 41) correctly states that "the presence of a cell R_2 is a tribal character, not generic as considered by Becker," he has in all his papers taken the view that this character is not even generic in the genus *Gonomyia*, and for this reason he regards *Lipophleps* as a subgenus of *Gonomyia*. I have previously given the reasons why I cannot agree with this point of view; still less can I agree with Alexander's opinion that species with a fundamentally different structure of the male genitalia can be congeneric.

4. Edwards has informed me that the figure of the wing of his *Thaumastoptera aldabrensis* is incorrect in several respects, that the venation in fact is the same as in the genus *Ptilostena* Bergr., save that R_3 is not turned upwards, and that the male propygium

of *aldabrensis* is almost quite identical with that of the type of *Ptilostena*. He therefore places *aldabrensis* without hesitation in *Ptilostena* which he thinks was correctly placed near *Gonomyia*. The upturned R_3 is only a specific character of *Pt. recurvinervis* Bergr.

5. Alexander has for some time placed *Empeda* as a subgenus of *Erioptera*. In this he differs widely from Schiner and Osten-Sacken who never even thought of the possibility of associating these two genera. I cannot help thinking that this innovation has very little to recommend it. In the paper where this new place was assigned to *Empeda* no reasons for the change were given, but later, speaking of *Erioptera brevior* Brun. (which is an *Empeda*), Alexander summarily says: "*Empeda* is merely an *Erioptera* in which the fusion of R_{2+3} is a little longer than usual." This fusion is, however, *much* longer in *Empeda*, so much in fact that, in opposition to *Erioptera*,¹ the radial cross-vein joins R_2 before its branching and that the cell R_2 assumes a quite different shape. And what of the other differences? Sc_2 is in *Erioptera*¹ removed very far backwards from the tip of Sc_1 , whereas it is placed close to its tip in *Empeda*; the middle legs in *Erioptera* are strikingly shorter than the two other pairs, which is not the case in *Empeda*; and so on. Osten-Sacken regarded *Empeda* as very closely related to *Gonomyia*, and it comes in fact at least as near to this genus as to *Erioptera*. In *Erioptera* he included several heterogeneous elements, and to add *Empeda* to them is only to increase the confusion. " Cu_1 tending to turn toward the apex of the wing" is given as a character of *Erioptera* in Alexander's key to the genera (Proc. U. S. Nat. Mus. 44, p. 492). This character fits some species of *Empeda*, but not all. If we unite *Empeda* with *Erioptera* and consider the fact that the length of Sc is rather variable in *Empeda*, and that the presence or absence of the cross-vein r is not perfectly constant as a generic character, we could quite as well unite *Gonomyia* with *Erioptera* and say: "*Gonomyia* is merely an *Erioptera* with the radial cross-vein wanting." The wing-venation of *Ptilostena aldabrensis* Edw. must be very similar to that of *Gonomyia blanda* O. S. (figured by Needham), and if the structure

¹ Excluding *E. imbuta* Wied. which forms the very distinct monotypic genus *Chilotrichia* which comes nearer *Empeda* than any other Eriopterine genus and in no case can be referred to *Erioptera*.

of the propygium, as Alexander seems to think, is not of much importance as a generic character, we cannot separate *Ptilostena* from *Gonomyia*. In wing-venation and other characters *Sacandaga* Al. seems to differ very little from *Erioptera* (in Alexander's sense), although the author assures that the resemblance is probably merely accidental" (whatever that may mean) and that they are different "in general appearance." We would then have together there the whole series—*Erioptera-Ilisia-Mesocyphona-Chilotrichia-Empeda-Sacandaga-Gonomyia-Ptilostena-Lipophleps*—happily united in the same genus. But would this really clear up the study of the Eriopterinae? In my opinion it would decidedly be a step backward. It seems much better to follow Needham, Edwards, and many other dipterists in regarding Osten-Sacken's so-called subgenera of *Erioptera* as distinct genera. They are much more sharply separated from each other than the subgenera (or rather groups) of *Limnophila* are, and there is scarcely any species of *Erioptera* (in its broad sense) that could not easily be determined as to its place in any of these genera. Of course certain species can in one or other respect approach towards some related genus, but this occurs in numerous genera in the whole animal kingdom without involving the necessity of giving up these genera. Lumping of genera is certainly in some cases better than the excessive and often absurd splitting so characteristic of Theobald's and Enderlein's writings, but it should not be done without adequate grounds. It must be admitted, however, that the Eriopterinae leave a more open field to personal opinions than most other groups of Tipulidæ.

6. *Acyphona* O. S. must take the older name *Ilisia* Rond. (1856). Rondani after the description expressly states that *Erioptera maculata* Meig. is the type, and this was the only species he referred to the genus. That he many years later placed also two other species in it cannot, of course, invalidate its claim to recognition in its original comprehension. In describing two Japanese species of this genus Alexander says (Canad. Ent., 1913, p. 287) that *maculata* Meig. is the only described palearctic species of the genus, but the north-european *obscuripes* Zett. and *areolata* Siebke belong there, as shown by Wahlgren and Lundström.

7. Brunetti's book on the Indian Nematocera, which on a cursory examination makes such a good impression by the detailed

descriptions and excellent figures, reveals on closer scrutiny a great many partly almost incredible mistakes, the most curious of which concern what he calls *Gnophomyia* O. S. and which is not a genus at all, but a mixture of heterogeneous forms belonging to several widely different divisions. It is unnecessary to publish my numerous notes on his Tipulidæ, as they are almost identical with the remarks quite recently made by Alexander (Ins. Insc. Menstr., 1913, p. 118-120).

8. In my above-quoted paper (p. 584) I said, "Meigen did not state in his paper [of 1800] that he accepted the binominal nomenclature, and there is nothing in the paper indicating that he did so. Admittedly generic names in works of this class cannot be taken into consideration." Bezzi (in a letter to me) objects to this, and I now find that the same objections have been made already by Coquillett (Canad. Ent., 1908, p. 457) who wrote: "There is, first, the name of the proposed new genus in proper Latin form, then a description of the genus, followed by a statement of the number of species known to the author as belonging to the genus." All this is true, save that Meigen did not state the number of species for all his genera, but when Coquillett goes on to say that "the author, therefore, had a correct idea of binomial nomenclature, and, so far as he went, he applied it in this paper," we must take exception to this conclusion. What has Meigen's statement of the *number* of species belonging to some of his genera to do with *binominal* nomenclature? There are other authors (after 1758) who, though well aware of the binominal nomenclature, did not accept it but continued to designate their species by short diagnoses instead of specific names. Among these authors was even a fellow-countryman of Linnæus himself, who described many new species of insects without specific names. Briefly, the facts are these: we know that Meigen accepted the binominal nomenclature in 1803 and 1804, but we do not know if he did so in 1800, and the code forbids the recognition of names in such works. If we do accept these names, we can as well go back to the genera of the pre-Linnean period and revive them. To say the least, there are no cogent reasons why these names should be accepted, and such reasons failing we can safely follow Meigen's own lead and quit them for ever.

9. Some authors—among American dipterists, Johnson and Alexander—have adopted the principle that if two (or more) species have been described in the same genus under the same specific name, the species described later must be renamed *even if* the two species now belong to quite different genera. Johnson has, for instance, given a new name to *Oropeza annulata* Say., because it was described as a *Tipula*, and there is a *Tipula annulata* of Linné, which now is referred to *Discobola*, a widely different genus. Had Say. wrongly cited Linné's species as being the same as his own, the change would be just, but now it is quite unnecessary, and the general acceptance of such a principle would lead to disastrous consequences. Between 1758 and 1830 a very great number of species was described, but referred to a comparatively very small number of genera. The consequence of this is that the number of specific names preoccupied in this sense is immense, not in Diptera, as so many genera were founded as early as 1803, but in many other groups, as Lepidoptera and Mollusca. If all classes of animals are considered, the number of species, which must be renamed according to this rule, amounts to thousands. Kirkaldy's catalogue of the Pentatomidæ and some of his minor articles, with their sweeping and useless alterations of specific names, are a good warning in this respect; fortunately almost no hemipterist seems to have taken any notice of his new names of this class. As Sherborn's bulky volume, "Index animalium"—a result of many years' work—only includes the species described till 1800, and few persons have time and liking for looking up names preoccupied in this sense, it is a matter of course that the eventual new names alleged to be necessary in such cases could be introduced only little by little. In the meantime not even our most familiar names of animals could be safe from a sudden rebaptizing. As a confusion of two species bearing the same specific name, but belonging to different genera, is out of the question, a change of names in such cases serves no purpose, and it is sincerely to be hoped that zoölogists who have accepted the contrary principle would reconsider the position they have taken in reference to this matter. It is true that we then have to restore some of the older names now standing as synonyms, but the number of these names is very small as compared to the names of the other class.